First Review

REVIEWER 1: *The authors endeavour a very interesting Bayesian replication of Jiang and colleagues (2006), in which the original research team used null hypothesis significance testing (NHST) and found that interocular suppression using the Posner cuing paradigm provided evidence for subliminally directed attention to gender-of-interest naked images. The current authors use continuous flash suppression and provide initial, and suggest that they will provide conclusive, evidence that even if the Jiang and colleagues (2006) effect occurs, it is not in response to invisible/imperceptible/subliminal attentional direction. Their Bayesian statistical framework is excellently explained and – with a "young passion" and "experienced research confidence" that make me feel that both very promising and very senior researchers were involved in this work – their analyses is applied and will be applied to show this outcome.*

I believe that the choice of replicating the Jiang and colleagues (2006) paper is an excellent one. I will start with what I find dire limitations in the original paper, that I believe the current authors could improve on.

RESPONSE: We appreciate the reviewer's positive assessment!

REVIEWER 1: *Firstly, Jiang and colleagues confure the reader with their population sample. At first they quote:*

"Results from 10 male and 10 female heterosexual participants
revealed that invisible images did influence the distribution of
attention."

and then

"Ten heterosexual men and 10 heterosexual women
participated in experiment 1. Ten gay men (an average score of 5.6
on the 7-pointKinsey scale; 0 is exclusively heterosexual, 3 is equally
heterosexual and homosexual, and 6 is exclusively homosexual) and
10 gay bisexual women (with an average Kinsey score of 4.5)
participated in experiment 2"

It takes a lot to see that these two population samples are used in different experimental stages, there is no mention of whether the second sample of heterosexual participants was the same (or not) with the first stage and of course there is no mention of statistical power, which, anyway, would embarrass the paper with a less than optimal power co-efficient (e.g. $P_{(1-\beta)} \geq .8$*).*

Under this light, the replication could be arranged to amend this problem and improve the reliability of the results. I am sure the last author knows significantly more than me about Bayesian power, but I could simply contribute a few references as a courtesy:

Kruschke, J. K., & Liddell, T. M. (2018). The Bayesian New Statistics: Hypothesis testing, estimation, meta-analysis, and power analysis from a Bayesian perspective. Psychonomic bulletin & review, 25, 178-206.

Halsey, L. G. (2019). The reign of the p-value is over: what alternative analyses could we employ to fill the power vacuum?. Biology letters, 15(5), 20190174.

Dziak, J. J., Dierker, L. C., & Abar, B. (2020). The interpretation of statistical power after the data have been gathered. Current Psychology, 39, 870-877.

Stein, T., van Gaal, S., & Fahrenfort, J. (2023). How (not) to demonstrate unconscious priming: Overcoming issues with post-hoc data selection, low power, and frequentist statistics. Low Power, and Frequentist Statistics.

And of course:

Vadillo, M. A., Malejka, S., Lee, D. Y., Dienes, Z., & Shanks, D. R. (2022). Raising awareness about measurement error in research on unconscious mental processes. Psychonomic Bulletin & Review, 29(1), 21-43.

RESPONSE: Fortunately as we are using Bayes factors, the extent to which the results justify asserting H0 is provided by the Bayes factor itself – power per se is irrelevant once the data are in. We have also estimated that our maximum N should be enough to establish an evidential Bayes factor, whether in favour of H1 or H0. Power is a frequentist concept, not a Bayesian one. The reviewer is entirely right to point out that Jiang et al did not determine power which renders their conclusions, for example about priming on conscious trials, groundless; and the reviewer is quite right we should address this deficiency, which we have done by using Bayes factors with an adequate maximum sample size.

REVIEWER 1: *Also, as a topical researcher, I think jiang and colleagues (2006)* ***explicitly*** *confuse responses before conscious awareness:*

"However, it makes ecological and evolutionary sense if important events can influence observers' spatial attention even before the observer becomes aware of the event."

With responses ***without*** *conscious awareness:*

"Recent studies have shown that subliminal presentation of emotional stimuli can modulate activity of the amygdala (4, 5), a subcortical nucleus that is centrally involved in emotional information processing."

This is a very common mistake, and perhaps the authors could benefit from the "ever-so-wise" literature that makes a distinction between these too, and dedicate a passage to this "oldest of fallacies":

Bargh, J. A., & Morsella, E. (2008). The unconscious mind. Perspectives on psychological science, 3(1), 73-79.

Bargh, J. (2017). Before you know it: The unconscious reasons we do what we do. Simon and Schuster.

Bargh, J. A., & Hassin, R. R. (2021). Human unconscious processes in situ: The kind of awareness that really matters. The cognitive unconscious.

I have also written on that, but Bargh makes a much better argument for it, so I would go with him if you would like to address this issue.

RESPONSE: The reviewer is right that there is an interesting distinction between our attention being drawn to a naked body before we consciously perceive the stimulus; and our attention being drawn to a stimulus we never consciously perceive. Jiang et al appear to establish the latter, stronger, case. We have now added a sentence describing this distinction.

REVIEWER 1: *Finally, concerning Jiang and colleagues (2006), I was always in awe of their statistical reporting:*

"(positive attentional effect, t9 = 7.08, P = 0.0001), (negative attentional effect, t9 = 2.41, P = 0.04) and although "they did not show a significant attentional effect to invisible nude female pictures (t9 = 0.85, P > 0.4)" (I am not sure what format p < .04/ p > .04 is (!!!) " F1,36 = 32.3, P = 0.0001"

I am not sure where they are getting their α thresholds (e.g., .04) and why there is not a single mention of effect sizes in the entire paper (e.g. Cohen's d or η^2_p). I am pro Bayesian only replications and maybe now it is the time for the transition from NHST to Bayes ***but*** *in this specific case – since the NHST statistical reporting is at best inadequate in the original – the authors might want to consider an Appendix section with proper reporting of NHST as well. After all, in my book, Jiang and colleagues (2006) failed to justify – even with NHST – their infamous conclusion:*

RESPONSE: t and p-values for subliminality testing have been added. For the other tests we report, the t and p values were included in all the tables.

REVIEWER 1: *"These results* ***clearly show*** *that spatial distribution of observers'*
attention can be modulated by the presence of certain types of
visual images even when the images are interocularly suppressed
and invisible."

Now, I find the current paper excellent, and I can only try to contribute to its improvement. Concerning the current paper then, in the abstract:

"…Observing attentional biases induced by visual stimuli below conscious

threshold is one way of providing evidence…"

Do you mean:

"…Observing attentional biases induced by visual stimuli below ***the*** *conscious threshold is one way of providing evidence…" or "…below the threshold of conscious awareness is …"*

RESPONSE: We have revised using the first suggested option.

REVEIWER 1: *I am sorry the text was available from the repository without lines, so bear with me while I am trying to offer my help. In page three, in reference "(Cohen, et al., 2012)", I believe you have one too many commas. This happens again (e.g., "MacLeod, et al., 2002") and then it does not happen in other occasions "(Wagenmakers et al., 2017)". I am a bit confused, but my understanding is "(Evil-subliminal-researcher et al., 1666)". I would say choose one format, if you know something I do not, and the extra comma is somehow correct.*

RESPONSE: The reviewer is quite correct, the extra commas have been removed.

REVIEWER 1: *Same page: "These approaches assume…" maybe "approaches" per se cannot assume, would you prefer "These approaches have been used to suggest" or a similar rephasing?*

RESPONSE: Revised as suggested.

REVIEWER 1: *References with three authors, such as "(He, Cavanagh, & Intriligator, 1996; Pitts, Lutsyshyna, & Hillyard, 2018)" can be written according to the new APA manual as "(He et al., 1996)" when in brackets, or "He and colleagues (1996)" when in text, if that helps you save word count in the paper.*

RESPONSE: The citation issues have been addressed.

REVIEWER 1: *In this passage, "…the functional nature of the different "access consciousness"" do you mean "…the functional nature of different accessibility levels to conscious awareness"?*

RESPONSE: This was a direct citation of the words used by Block (1995), therefore no change has been made on this.

REVIEWER 1:
"Whether the evidence for phenomenal consciousness rules out attentional involvement is debatable (Jennings, 2015; Phillips, 2011), it remains an influential idea (Cohen et al., 2012; Noah, & Mangun, 2020)." Are you missing a "but"?

*"Whether the evidence for phenomenal consciousness rules out attentional involvement is debatable (Jennings, 2015; Phillips, 2011), **but** it remains an influential idea (Cohen et al., 2012; Noah, & Mangun, 2020)."*

Struggling to make sense without the "but" there.

RESPONSE: "but" added

REVIEWER 1: Similarly, "back-masking" is a tad odd, maybe the more traditional "backward masking" phrasing makes more sense?

RESPONSE: The word "back-masking" has been changed to backward masking.

REVIEWER 1: *There are a few instances like these above throughout the text. Would you care to proofread carefully and make your most interesting ideas more verbose and grammatically, syntactically, orthographically precise/correct please?*

I do not wish to be pedantic, it is clear as day that your English is exceptional, so I will stop going point-by-point and leave it to you to apply your skills to improve the language in the text.

"In control trials in which the colour patterns were replaced by an identical pair of nude

images to the other eye, so that the images were conscious, the attention modulation was non-significant."

Concerning this argument, I am (again…) at awe that the subtext here is "unconscious priming works, conscious doesn't" but if you would like to go deeper into it and comment it see:

Lapate, R. C., Rokers, B., Li, T., & Davidson, R. J. (2014). Nonconscious emotional activation colors first impressions: A regulatory role for conscious awareness. Psychological science, 25(2), 349-357.

And the rest of Regina's (Lapate) work that – although I disagree with – makes a very similar argument.

RESPONSE: The possible explanation provided by Lapate et al. has been added to the discussion.

REVIEWER 1: *Hmmm… let's peruse together this passage: "On the theory that homophobic heterosexual men are unconsciously attracted to naked men, this group should show attentional attraction to naked male images; nonhomophobic heterosexual men will not show such attraction but rather repulsion."*

If you provide evidence that although d', A', A" or A was not significantly different to chance-level perception, but you ***prove*** *with Bayesian evidence that they were not at-chance (i.e., BF > .3) wouldn't that mean any self-report was conscious and, therefore, subject to self-presentation sexual biases? What are you expecting if the responses involve conscious perception or meta-cognition (etc.)? Perhaps, any kind of psychophysiological assessment could "reveal" the truth here beyond self-reports, or if this is not possible to include a discussion of self-reports vs psychophysiology is in order? For example, have a read at this, where the effect I am discussing is very-very clear:*

RESPONSE: Unfortunately we will not be in a position to assess the biases of homophobic people because there will not be enough coming up on our measure.

REVIEWER 1: *Leong, M. Q., Yu, Z., Tsikandilakis, M., & Tong, E. M. (2023). "See no evil. Feel no evil?": Exploring emotional responses to masked moral violations in religious and nonreligious Singaporean participants. Evolutionary Behavioral Sciences.*

This is a – psycho-philosophically – very hard passage to write. I have highlighted it in many other papers and the reviewers asked for it more explicitly and in more depth. Mind the conservative reviewer here; they will ask for a distinction between self-reports, and implicit measures of arousal and repulsion. I am attaching some more papers where I addressed the subject to help you out. I am doing it to help you write this passage as best I can (though you are the better writers!). I am not doing for references:

Tsikandilakis, M., Bali, P., Derrfuss, J., & Chapman, P. (2020). "I can see you; I can feel it; and vice-versa": consciousness and its relation to emotional physiology. Cognition and Emotion, 34(3), 498-510.

Tsikandilakis, M., Leong, M. Q., Yu, Z., Paterakis, G., Bali, P., Derrfuss, J., ... & Mitchell, P. (2021). "Speak of the Devil… and he Shall Appear": Religiosity, unconsciousness, and the effects of explicit priming in the misperception of immorality. Psychological Research, 1-29.

Yu, Z., Bali, P., Tsikandilakis, M., & Tong, E. M. (2022). 'Look not at what is contrary to propriety': A meta-analytic exploration of the association between religiosity and sensitivity to disgust. British Journal of Social Psychology, 61(1), 276-299.

"Therefore, the theory concerning homophobia was not testable.
Indeed, it may not be testable even with a much larger N drawn from University of
Sussex undergraduates."

This is good, but it is speculative. I do think a discussion is in order, maybe visit the texts above and feel free to "borrow" a few ideas with my permission and my blessings!

RESPONSE: We thank the author for these references which we will bear in mind for future research. The reviewer's papers may provide a starting point for a way for examining implicit homophobia (or implicit attitude toward non-heterosexuality), using physiological responses, but this goes beyond what we can achieve in the current experiment.

REVEWIER 1: *“On the theory that the CFS rendered knowledge of the side subliminal, then even if*
subjects are above objective threshold, they should be at subjective threshold in
indicating which side the image was on.”

I would be very happy to help you with this in further reviews if you decide to follow this method! If you do the latest I have written on this one is a direct replication manual for this method ad I would be very happy to assist as a reviewer if you attempt it:

Tsikandilakis, M., Bali, P., Karlis, A., Mével, P. A., Madan, C., Derrfuss, J., & Milbank, A. (2023). Unbiased Individual Unconsciousness: Rationale, Replication and Developing Applications. Current Research in Behavioral Sciences, 100109.

RESPONSE: We thank the reviewer for this paper which we will certainly bear in mind for future studies.

REVIEWER 1: *I need to say that I had super-giga problems with APA images in the Appendix (they would not let me show them), and particularly in APA journals where not only they would not let me show them, but they also additionally asked for written and signed permission that I was eligible to use them. So please be prepared to receive “a bureaucratic attack” for every picture presented after page 31 in any most (all?) journals this fine work will be submitted to.*

RESPONSE: We have removed the images from the paper and uploaded them in a folder of the current OSF project.

Second Review

REVIEWER 2: *It was a delight to read this pre-registration. I was happy to see that it came with pilot data, and that all analysis steps were well thought out, following the best guidelines at our disposal to date in the field of consciousness science. This is an example for the field.*

RESPONSE: We are grateful for the reviewer's positive appreciation!

REVIEWER 2: *I do have some suggestions / recommendations, in particular relating to establishing unconscious processing.*

First, instead of testing whether the magnitude of their priming effect is larger than zero, the authors plan to test whether it is larger than the 'priming effect' that would be expected by regression to the mean alone (following Shanks' approach). Thus far, however, I don't recall having seen (m)any effect(s) in the literature convincingly passing this test; although I am happy to be convinced otherwise. I therefore fear that, in this regard, the pre-registered analyses might be too conservative, and wonder whether a null effect would be interpretable as evidence of absence.

RESPONSE:

The problem is one raised by David Shanks, but the solution we are using is not one provided by Shanks, who did not offer a solution for the post hoc selection of trials categorized (e.g. as seen or unseen) – his solution was for selection on the basis of continuous variables. Thus, we are using the solution proposed by Dienes (2022), used in Skora et al. (2023) and Jurchis and Dienes (2023). In Skora et al the correction showed that the maximum regression to the mean effect was so small no correction was necessary; and in Jurchis and Dienes, the evidence for implicit learning survived the correction. So this correction is not so conservative it rules out finding implicit effects. (It could rule out finding such effects if the proportion of conscious trials were getting close to 50%, so we will endeavour to keep the proportion of conscious trials much lower than that).

REVIEWER 2: *Second, from another perspective, the described approach might be too liberal. To replicate the original effect, the researchers would need to (1) establish invisibility of the primes, and (2) observe a significant priming effect. Following the logic of Meyen and colleagues (2022), however, we would succomb to the interaction fallacy were we to interpret* *this as evidence for unconscious processing; that is, the fact that effect 1 is significant, and effect 2 is not, does not mean that the difference between these effects is significant (the same holds true for a Bayesian approach). Thus, a key analysis to include would be the paired comparison of the direct task with the indirect task (i.e., is the priming effect stronger - in standardized units - than the visibility of the primes?).*

> Meyen, S., Zerweck, I. A., Amado, C., von Luxburg, U., & Franz, V. H. (2022). Advancing research on unconscious priming: When can scientists claim an indirect task advantage?. Journal of Experimental Psychology: General, 151(1), 65.

RESPONSE: The reviewer is correct that one needs a principled method for determining the performance expected on the direct test given the performance on the indirect test. Meyen et al. provide a neat method, but it assumes equal signal to noise ratios on both tests, an assumption that is certainly false in general. Dienes (2015) put the problem this way: What performance on the direct test would one get for a given amount of priming - in a situation where the perception was conscious? That is, one empirically determines the mapping between the direct and indirect tests in a condition known to be conscious (then one does not have to make any a priori assumptions about the signal to noise ratios of each test). (This procedure was put into practice by Skora et al., 2023.) We had decided against this approach on the grounds there may be no priming in the conscious condition (at least if we support Jiang et al.'s stated conclusion). But the reviewer's point has prompted us to reconsider this. We will now add a pre-registered test for priming in the conscious condition. If we find evidence for priming in the conscious condition, we will use the method of Dienes (2015) to determine the direct performance expected if the priming in the nominally unconscious condition were conscious, and that value will inform the model of H1 for the Bayes factor.

REVIEWER 2: *Some very minor points:*

In the results of the pilot (page 13) it is clearly stated how many participants are excluded, but not how many are included in the analysis.

RESPONSE: The number of participants included has been added.

REVIEWER 2: *It is unclear whether the stastistical tests reported about the pilot data already compare the priming effect to the effect of regression to the mean or compare the priming effect to zero.*

RESPONSE: As we did not find evidence to support conscious priming effects, the priming effect, as defined in the analyses section, was compared to 0. We have added a sentence to make this explicit.

REVIEWER 2: *Page 21, bottom paragraph states "trails" instead of "trials".*

RESPONSE: Revised.

REVIEWER 2: *The Study Design Template should probably be rotated by 90 degrees in landscape orientation, because the narrow columns imposed by portrait orientation make it very difficult to read.*

RESPONSE: Format adjusted.